Art. VII.—*Influence of Light on the Development of Bacteria.*

By J. Jamieson, M.D.

[Read 8th June, 1882.]

It is a common opinion, and probably a correct one, that abundance of light is favourable to the preservation and restoration of health. In how far the evil effects, resulting from the occupation of badly lighted dwellings, are due to the want of light in itself or to other insanitary conditions, damp, bad drainage, dirt, &c., which are often associated with it, is not easy to prove with certainty. It has been supposed, further, that the spread of epidemic and other contagious diseases is favoured by conditions, which prevent the access of the sun's rays to the walls and to the interior of ordinary dwellings, and still more of hospitals, This unfavourable result of shutting off direct sunlight has even been ascribed to the effect of that light in destroying disease germs. Very much of all this is simply matter of opinion, the supposed destructive action of sunlight on germs being, perhaps, assumed from the common observation, that the various species of mould grow and multiply most freely in close, dark, damp places. Even here, however, I am not aware of exact observations or experiments having been made to test the share that darkness, by itself, without the other conditions, may have in favouring mouldy growths.

Confirmation of the common opinion about the destructive action of sunlight on those low forms of life, with which the germs of some diseases are probably closely allied, seemed to be supplied by the investigations of Messrs. Downes and Blunt, reported in detail in the "Proceedings of the Royal Society of London," for 1877 (vol. xxvi., p. 488). The general conclusions to which they had come were summarised in a short communication in *Nature*, for July 12th, 1877, to the following effect:—Light is inimical to the development of bacteria, and may either prevent or only retard their development; but that, for the attainment of the full effect, direct insolation is necessary. The germs originally present are destroyed by direct insolation, while the fitness of the solution in which they were contained, to serve as a nidus, is not affected. They used Pasteur's solution, inoculated with bacterial germs, and then exposed to direct sunlight in test tubes. The experiments described seemed to bear out their contention, though the results were not of a uniform character. They found an exposure of 3½ hours suffice for sterilization in one case, while in another it was not produced after 11 hours. They could suggest no other explanation than "that external conditions—notably temperature—may retard or counteract the preservative quality of the solar rays." Remarkably enough, they found that in weak solutions, diluted to one-tenth, they failed to accomplish sterilization. Professor Tyndall read a communication before the Royal Society on the same subject (Proceedings, vol. xxviii., p. 212), in which he stated that when flasks, containing infusions of cucumber and turnip, were inoculated and exposed to the sun, they were not completely sterilized, as they showed abundant formation of bacteria after they were removed to a warm room. In view of the anomalies which had been met with by Messrs. Downes and Blunt, and the different conclusions he had arrived at, he suggested the necessity for repeating the experiments. In the same volume of the Proceedings (xxviii., p. 199), there appeared another paper by these gentlemen, extending and confirming their conclusions. Finally, at the meeting of the British Association in 1881, Professor Tyndall read a paper (*Nature*, Sept. 15th, 1881), in which he gave the results of another series of experiments. He found the statements of Messrs. Downes and Blunt correct, in so far as the suspension of development was concerned, but he never succeeded in producing perfect sterilization, all the flasks exposed to sunlight becoming turbid when removed to a shady place. He expressed the definite opinion that the difference between flasks exposed to the sun, and those kept in the shade, after inoculation, was not owing to difference of temperature. It seems to have been tacitly assumed, both by him and by the other investigators, that any elevation of temperature, to which their tubes and flasks were liable in the course of their exposure, could only be favourable to bacterial growth, and merely noting this fact, I go on to relate my own experiments, which have brought me to different conclusions.

I was led to make them by the discussions going on as to the sanitary condition of the Melbourne Hospital, and the injurious effects supposed to have been produced by the comparative exclusion of the sun's rays from parts of the buildings. Though my investigations have not led me to conclude that light is inimical to the development of bacteria, I by no means wish to derive therefrom the further conclusion, that it is a matter of indifference whether or not hospital wards, or other human habitations, are well lighted. I do think it probable, however, that insufficient lighting does not act, by allowing the free growth of disease germs, and so favouring the origination or spread of erysipelas and allied diseases.

In the experiments, now to be described, I used Cohn's solution, as in a series of investigations on the action of disinfectants, already communicated to this Society (11th October, 1877). This fluid, admirably adapted for the cultivation of the *Bacterium termo*, the active organism in the production of putrefaction, has the following composition:—

Tartrate of Ammonia ...	2
Sulphate of Magnesia ...	1
Acid Phosphate of Potash	1
Chloride of Calcium ...	$\frac{1}{10}$
Distilled Water ...	200

My ordinary procedure was to put about two fluid drams of this solution into ordinary one-ounce phials, and, after inoculation, plug them with cotton wadding. Free access of air was thus allowed, while solid particles were excluded. A considerable series of experiments, sixteen in number, were made to determine—(1) Whether ordinary diffused light interferes in any way with the development of bacteria in Cohn's solution; (2) whether direct insolation has that effect; and (3) whether direct insolation quickly causes the destruction of bacteria in the dried state. They were begun in February last, and continued as other occupations permitted.

Exp. I. On February 21st three phials, inoculated each with three drops of putrid meat juice swarming with bacteria, were placed outside, on the sill of a window on which the rays of the sun fell nearly all day. The weather was very hot. On the 23rd all were still quite transparent, and one was removed and put in a shady place. On the 26th this showed three specks of mould, but no opalescence from bacteria. On the 28th the others left in the sun were still perfectly transparent, and showed no mould formation.

Exp. II. On 28th February, at 11.40 a.m., an ounce of solution was inoculated with twenty drops of putrid meat juice, and distributed in four bottles. Two were exposed to the sun, and the others, wrapped in brown paper, were placed alongside of them. The weather was bright but cool. On 3rd March both of the covered bottles began to show cloudiness, and soon became quite opalescent. Next day (4th), at 2.30 p.m., both of the exposed bottles were quite transparent. One of them was then wrapped in paper, and both left in the same place, but on the 6th they were still transparent.

So far these results seemed fully to confirm the conclusions of Downes and Blunt. Direct insolation had not only checked the growth of the bacteria, but had actually sterilized the solutions so far as they were concerned. The survival of mould spores, after the destruction of bacteria, also agreed with what these observers had found.

I proceeded next to try what the effect of diffused light would be.

Exp. III. On 11th March, at 2.30 p.m., I inoculated six drams of solution, with five drops of opalescent fluid from one of the bottles left from a previous experiment, and distributed it equally in four bottles. Two were wrapped in brown paper, and the others left uncovered, and all placed in a bright light on an inner window sill, but guarded from the direct rays of the sun. On the 13th, at 9 a.m., they were all nearly opaque, no difference being perceived. It was evident from this, that bright diffused sunlight is not inimical to the development of bacteria. This experiment, conclusive enough in itself, was confirmed by the next.

Exp. IV. An ounce of solution, inoculated with four drops of opalescent fluid from previous experiment, was put into four bottles. Two were exposed to the sun; one in the same situation but wrapped in brown paper, and the fourth left exposed to the light inside, at 2.30 p.m. on 15th March. The temperature in the sun was noted at 110° F., and next day at 112° F. On the 17th, at 9 a.m., the wrapped bottle and the one in diffused light were already cloudy, the latter most distinctly. The two exposed bottles were perfectly transparent, and both remained so till the 19th, at noon, though one of them had been taken out of the sun.

Having apparently established the fact that the bacteria in Cohn's solution may be not only retarded in their development, but even killed by exposure to the sun's rays, I tried next to discover the time needed for their destruction.

Exp. V. On 27th March, at 11.30 a.m., four bottles charged with solution, inoculated as in Exp. IV., were taken; one of them left in ordinary diffused light for a test, and the other three placed in the sun, and left for $1\frac{1}{2}$, $2\frac{1}{2}$, and 5 hours respectively, and then put beside the test bottle, the thermometer marking 116°, 124°, and 108° F. at different times in the course of exposure. On the 30th, at 9 a.m., the test solution was found to be milky and crusted; those exposed for $1\frac{1}{2}$ and $2\frac{1}{2}$ hours showed traces of opalescence, while that which had been exposed for five hours was quite transparent, remaining so till the morning of 1st April, when it began to show slight opalescence; the others, before that time, having become almost opaque. With the conditions under which I experimented, therefore, five hours proved almost sufficient for the sterilization of the inoculated solution.

I began now to ask myself in how far the effect, so clearly produced by insolation, might not be due to the solution being raised, by standing on a hot window sill, to a temperature sufficient to paralyse and even kill bacteria, and that independently of any chemical or other action of the sun's rays. The utter want of any such destructive influence in diffused light made this not improbable, and I altered my procedure in the next two experiments.

Exp. VI. On 6th April, at 2 p.m., the weather being bright but cool, three bottles, containing each two drams of inoculated solution, were suspended outside of a window, in front of the glass, with the same exposure. The 7th was cloudy, the 8th bright and cool, and on the 9th, which was bright and warm, all were still found transparent; and at 9 a.m. one was brought inside out of the sun. On the 10th, which was also bright, another was taken in at 9 a.m., the one which was left out then showing faint signs of cloudiness. A thermometer hung up beside it marked a temperature of 98° F. Next day (the 11th), at 9 a.m., the exposed bottle was quite milky, the others just beginning to show traces of opalescence, the one removed on the 9th being least advanced. Here then the solution which had been longest and continuously exposed to insolation became first altered by bacterial development. There was scarcely any explanation conceivable, but that, in all, the development had been retarded by the coolness of the weather at first; and that the warmth (98° F.) outside, on the 10th and 11th, favoured that development in the bottle exposed to it; the others, inside of the house, being at a lower temperature. Long and continuous insolation had here certainly been little, if at all, inimical to the growth of bacteria.

Exp. VII. On 14th April, at 12.30 p.m., I inoculated six drams of solution with two drops of bacterialised fluid, and divided it equally over three bottles. They were all suspended in the sun, one of them having been first wrapped in brown paper. The weather was cloudy and almost cold on the following days, the 19th and 20th, however, being bright all day; and only on the 21st were the exposed bottles found to be opalescent. The solution in the covered bottle was quite milky. My interpretation of these conditions was, that the coldness of the weather had checked the multiplication of the bacteria in the first days, growth only beginning actively in the brighter and warmer weather of the 19th and 20th. The more advanced development in the covered bottle was most naturally to be ascribed, I think, to the wrapping keeping it at a more uniform temperature, and especially preventing that from sinking so low during the night.

The result of these two experiments was clearly to show that insolation, associated with moderate or low temperature, has no destructive influence on bacteria, not even apparently retarding their growth. I was, therefore, driven to conclusions directly contradictory to those both of Professor Tyndall and of Messrs. Downes and Blunt. The doubt, of course, which at once suggested itself was, whether the sun's rays, even in summer in England, would raise a solution exposed to them to a temperature sufficient of itself to destroy bacteria. To settle this point it was necessary, first of all, to ascertain the lowest temperature at which the *Bacterium termo* is paralysed or killed. This information has been provided by the careful experiments of Dr. Eduard Eidam, reported in Cohn's "Beiträge Zur Biologie der Pflanzen" (heft. III., p. 208). He found that while very low temperatures check indefinitely the growth of this organism, growth becomes more active with gradual elevation up to 35° C. (95° F.). Temperatures above this are again less favourable, and between 40° and 45° C. (104°-113° F.), the bacteria remain in a torpid condition, a kind of heat rigidity (Wärmestarre), but are not killed. An exposure for seven days to a temperature of 45° C. was sufficent to cause their destruction; while fourteen hours of exposure at 47° C. (116.3° F.), three to four hours at 50°-52° C. (122°-125.6° F.), and one hour at 60° C. (140° F.) sufficed to produce the same effect. Under a hot Australian sun there is no difficulty about getting a temperature of 140° F. or over, 125° F. being quite common, and so the destruction of bacteria by insolation is easily accounted for. Whether a high enough temperature for that purpose is readily attained in England may be doubtful, and the fact that Professor Tyndall never succeeded in sterilizing his solutions, meets its explanation in this way. It is possible that, in June or July, when Messrs. Downes and Blunt carried on most of their investigations, a heat of 125° F. may be occasionally reached for three or four hours continuously, and this would suffice. An anomaly, to them apparently unaccountable, viz., that solution in very small test tubes was more easily sterilized than when contained in larger ones, may be explained by the circumstance that a small body of fluid would more speedily and certainly be raised to the required temperature than a larger one. The fact that Professor Tyndall, in his experiments, used flasks, which I presume were of considerable size, would on the same principle account for his failure to get complete destruction of germs—the attainment of temporary torpidity, by a temperature slightly exceeding 104° F., being comparatively easy.

While, therefore, it might be going beyond my competence to deny to direct sunlight any influence inimical to the development of bacteria, I have no hesitation in expressing the opinion that such inimical influence of light *per se* is not established, either by my own experiments, or by those which I have ventured to criticise, and to interpret in a different sense from their authors. I can explain their error only by supposing that it had not occurred to them as possible, that bacteria might be paralysed, or even killed, by continuous exposure to ordinary summer heat. An expression, contained in one of Messrs. Downes and Blunt's Memoirs, already quoted, to the effect "that temperature may retard or counteract the preservative quality of the solar rays," seems to show clearly that it was actually their opinion, that any elevation of temperature, to which their solutions were exposed, could act only by hastening the development to such an extent as to overcome the destructive power of light as light. Professor Tyndall says, "On many occasions the temperature of the exposed flasks was far more favourable to the development of life than that of the shaded ones."

When it is considered how much greater is the difficulty of destroying bacteria or their germs in the dry than in the moist state, either by heat or disinfectants, it might almost with safety be concluded that insolation, which fails to destroy the *bacterium termo* in solutions, is not likely to injure it when dried.

As reported in my previous communication to this Society, I found dried bacteria resist a temperature of about 212° F. for fifteen minutes, and, therefore, no solar heat could be expected to kill them. But as desiccation, when sufficiently complete, has that effect, it might readily happen that exposure to the sun's rays in hot weather might act destructively, in virtue of its drying effect. To test the influence of insolation on the dry bacteria, I soaked blotting-paper with bacterialised solution, obtained from a bottle used in one of the previous experiments, and exposed it to the sun freely suspended by a piece of thread. Similar pieces of paper were hung up in a shady but well-aired passage, and in a well-lighted room. This was done twice; and, to test the condition of the bacteria in the pores of the paper, the following precautions were taken:—Bottles, as before, after receiving about two drams of pure solution, were plugged with cotton wadding, and then kept for some time in boiling water to secure complete sterilization. After time was allowed for cooling, the plug was taken out, a little square of the blotting-paper dropped quickly in, contact only with scissors being allowed, and the plug replaced. In the first series of experiments, carried on in hot weather, it was found that, after two days, the bacteria had not been killed in any of the papers; that, after four days, they had been killed in that exposed to the sun, and that hung in a current of air, but in the shade; and not killed in that which had been suspended in bright, diffused light. After seven days, the last also failed to bring about milkiness in the solution. I conclude, therefore, that it was simply a question of desiccation with all of them, the time needed to produce destruction in that way varying with temperature and exposure to currents of air. In the other series, a similar result was reached. The growth of bacteria in the bottle containing the sun-dried paper was later in occurring than in the others, but was not completely prevented even after five days of exposure. The interest of these experiments consists in the proof supplied, that, under conditions very favourable to rapid and complete desiccation, such as free exposure to air and sun, bacteria may be destroyed in a comparatively short time, not less, however, than from two to four days being needed even in this climate in summer, and even longer, unless the weather be actually hot.

Since writing this paper I find from a passage in a letter contained in *Nature* (vol. III., p. 247), that Dr. Bastian had been led to ascribe to the actinic rays of the sun an important influence in promoting the spontaneous generation of organisms in organic infusions. Though that notion may be considered as fairly set aside by Professor Tyndall's experiments, recorded in the *Philosophical Transactions* (part I., 1877), and again in his *Essays on the Floating Matter of the Air* (p. 231), the interesting fact remains that, at different times, both a favouring and an inimical action on the development of these minute organisms should have been ascribed to the sun's rays, when in reality they appear to have little, if any, appreciable direct influence in either direction.

Art. VIII.—*Remarks on Railway and Marine Signals, and on the Necessity of Accurate Testing of the Sight of Signal and Look-out Men by Land and Sea.*

By James T. Rudall, F.R.C.S.

[Read 8th June, 1882.]

The great increase of travelling in recent years, the large numbers of ocean-going and other steamships, the frequency of railway trains running over the same lines, and the numerous intersections of these, have become attended by dangers of which some cannot be wholly eliminated; and others, though avoidable, are only now beginning to receive attention.

If one remembers that between New York and Liverpool nearly thirty large steamship companies have their vessels